OUR ASTRONOMICAL COLUMN.

Astronomical Occurrences in November:—

November 1.	5h. 17m. to 6h. 0m. Occultation of 103 Tauri (mag. 5·5) by the moon.
5.	4h. Mars 3° 41′ N. of the moon.
12.	Mars rises at 9h. and visible afterwards throughout the night in Cancer. Diameter of the planet 9″·6.
15.	Mars. Illuminated portion of disc = 0·900.
15.	Jupiter becomes visible as a morning star. Polar diameter 28″ 8.
15.	Meteoric shower from Leo in the early morning hours. (Radiant 149° + 23°.)
16.	7h. Vesta 10′ N. of the moon.
17.	10h. 11m. Minimum of Algol (β Persei).
20.	7h. Minimum of Algol (β Persei).
22.	7h. 9m. to 8h. 13m. Occultation of 19 Piscium (mag. 5·2) by the moon.
23–24.	Meteoric shower from Biela's comet. (Radiant 25° + 43°.)
25.	12h. Uranus in conjunction with the sun.
28.	14h. 7m. to 15h. 6m. Occultation of 103 Tauri (mag. 5·5) by the moon.
29.	11h. 19m. to 11h. 49m. Occultation of the star DM + 24°, 1033 (mag. 6·0) by the moon.

Comet Brooks.—A telegram from Kiel, dated October 21, informs us that at 7h. Geneva time, on October 20, Brooks found a comet in position R.A. 14h. 32m. 8s. and Decl. + 60° 26′. It was moving in a south-easterly direction, and was described as "bright." A later telegram, dated October 22, gives the position of the comet from an observation made at Pola on October 21, at 16h. 48·5m. Pola mean time; this was R.A. 15h. 4m. 12s. and Decl. + 57° 50′.

The comet was thus situated in the constellation of Draco, and moving to the south of the star β Draconis.

The Large Sun-spot.—In this column for September 8 we drew attention to the large spot that had a few days previously made its appearance on the eastern limb of the sun, and remarked that "the spot will be well worth watching during the remaining period of its visibility, especially as many years may perhaps elapse before observers are favoured with another spot of similar size."

At the present time there is another great spot, larger, perhaps, than the one referred to above, which is now beautifully visible near the centre of the solar disc. This very compact group, which is not situated very far from the solar equator, is composed of two large umbræ surrounded by rather irregular shaped penumbræ, with several small spots scattered around them. Another smaller spot is also following the large one.

It will be well for observers to look out for an aurora and magnetic storm when the spot passes the central meridian, as was the case when the spot, previously referred to, was in the same position on September 9 last.

The Harvard Astrophysical Conference.—It was on the occasion of the dedication of the Yerkes Observatory that the Americans held their first astrophysical conference. So great was the success of this, their first trial, that it was expected that more would be held in after years. For this we had not long to wait, and we have now before us a full account of the conference recently held at Harvard, the oldest observatory devoted to astrophysical research, contributed to *Science* (October 7) from the pen of Prof. M. B. Snyder. There could have been no more appropriate place of meeting in America for the second conference than that of the Harvard College Observatory, for Prof. E. C. Pickering's vast organisation of work in all modern branches of astronomy is second to none.

The conference was presided over alternately by Prof. J. R. Eastman, of the United States Naval Observatory, and Prof. Hale, of the Yerkes Observatory, and the meetings were not only held on August 18, 19 and 20, but were carried over to a series of adjourned meetings held during the course of the subsequent week.

The papers read were very numerous, and dealt with all kinds of astrophysical work. The work carried on at Harvard formed, perhaps, the chief item in the programme. Some of the papers dealt with were as follows:—

Prof. George Comstock, on "Some investigations relating to zenith telescope latitudes"; Dr. Harold Jacoby, on "Photographic researches near the pole of the Heavens"; Mrs. Fleming, on "Stars of the fifth type in the Magellanic clouds," which establishes another connection between these objects and the Milky Way.

Prof. Solon Bailey presented a paper on "Variable stars in clusters," which is a subject most interesting in the light of recent investigations.

We notice that general plans for observing the total eclipse of the sun on May 28, 1900, were briefly discussed, and a committee appointed to consider the work of organisation.

Another important question brought up at the conference was the creation of a permanent astronomical and astrophysical society. This proposal was formally accepted, and a committee, consisting of Profs. Hale, Comstock, Pickering, Newcomb and Morley, was appointed to consider the organisation.

THE KNIGHT-DARWIN LAW.[1]

THE law under the above title is known to botanists through H. Müller ("Befruchtung der Blumen," Eng. trans., p. 4), who says that Andrew Knight "laid down the law that in no plant does self-fertilisation occur for an unlimited number of generations." This he call's Knight's Law, and later, in substantially the same form, it becomes the Knight-Darwin Law. For the statement of Knight's Law the reader is referred to that author's celebrated paper: "An account of some experiments on the fecundation of vegetables" (Phil. Trans., 1799). The words, however, do not occur in Knight's paper, and I imagine that Müller got them from Charles Darwin's paper on the fertilisation of papilionaceous flowers, where occurs the passage (*Gardener's Chronicle*, 1858): "Andrew Knight many years ago propounded the doctrine that no plant self-fertilises itself for a perpetuity of generations."[2] The words are not given in inverted commas, and I strongly suspect that, with a singular lapse of his usual accuracy, my father was merely giving his own interpretation of the conclusion which seemed to flow from Knight's expressions when taken with the whole of the context. For in the "Effects of Cross- and Self-fertilisation," 1876, p. 7, he quotes Knight's actual words. After referring to Sprengel, he goes on: "Andrew Knight saw the truth much more clearly, for he remarks: 'Nature intended that a sexual intercourse should take place between neighbouring plants of the same species' . . ." and again: "'Nature has something more in view than that its own proper males should fecundate each blossom.'" Here we have simply the general statement that hermaphrodite flowers are not necessarily self-fertilised; a statement of fundamental importance in floral biology. If the positive statement that "no plant self-fertilises itself for a perpetuity of generations" is to be found elsewhere in Knight's writings, I think Darwin would have quoted it.

In the "Origin of Species" (edition i., p. 96) he refers to Knight in the following words: "Nevertheless I am strongly inclined to believe that with all hermaphrodites two individuals, either occasionally or habitually, concur for the reproduction of their kind. This view, I may add, was first suggested by Andrew Knight."

Lastly, in 1868 ("Variation of Animals and Plants," ii. p. 175), after speaking of his own hypothesis, "that it is a law of nature that organic beings shall not fertilise themselves for perpetuity," he adds: "This law was first plainly hinted at in 1799, with respect to plants, by Andrew Knight." If he had known any positive expressions—going beyond the nature of a hint—in Knight's writings, would he not have quoted them? It seems, therefore, that, as far as Knight is concerned, the law should be a general statement of the tendency to cross-fertilisation of hermaphrodites, and not the positive statement quoted by Müller.

When we pass from Knight's share in the law to Charles Darwin's—there are difficulties in fixing on the most authentic wording of the law. The earliest form is that occurring in the "Origin of Species" (ed. i. p. 97).

"These facts alone incline me to believe that it is a general law of nature (utterly ignorant though we be of the meaning of the law) that no organic being self-fertilises itself for an eternity of generations; but that a cross with another individual is occasionally—perhaps at very long intervals—indispensable."

In the sixth edition of the "Origin." 1872, he retains the above passage with the omission of the words "utterly ignorant though we be . . . law," and with the addition of a reference to his own experiments, *i.e.* to those on cross-fertilisation.

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[1] A paper read before Section K (Botany) at the British Association, 1898.

[2] This sentence is quoted by Müller, "Historical Introduction," p. 29.

This is the most strongly worded form of the Law, and one which is generally adopted. But shortly after the publication of the "Origin," *i.e.* in 1862, the Law took a vaguer form in the "Fertilisation of Orchids" (ed. i., 1862, p. 359), where he wrote: "Nature thus tells us, in the most emphatic manner, that she abhors perpetual self-fertilisation." This form of the Law is adopted in the "Effects of Cross- and Self-fertilisation" (p. 8), where he writes: "If the word perpetual had been omitted, the aphorism would have been false. As it stands, I believe that it is true, though perhaps rather too strongly expressed."

The aphorism is clearly not a literal statement of fact, and in describing it as "true," he probably meant that perpetual self-fertilisation is very strongly and very generally guarded against in nature. For he well knew that "some few plants seem to be invariably self-fertilised" ("Cross- and Self-Fertilisation," p. 3). With regard to these cases he makes the just remark: "These exceptions need not make us doubt the truth of the above rule, any more than the existence of some few plants which produce flowers, and yet never set seed, should make us doubt that flowers are adapted for the production of seed and the propagation of the species."

It is only fair to add that this argument also occurs in the "Variation of Animals and Plants under Domestication" (vol. ii. p. 91, 1868), and was therefore of considerably earlier date than his book on "Cross- and Self-Fertilisation" (1876).

To sum up:

(1) If the expression, Knight-Darwin Law, is to continue in use, it ought to be applied to a statement on which Knight and Darwin are undoubtedly agreed, *i.e.* that "nature intended that a sexual intercourse should take place between neighbouring plants of the same species."

But the name of Knight-Darwin Law is now firmly associated with the positive statement "that no organic being fertilises itself for an eternity of generations," and it would be useless to suggest a new nomenclature.

(2) If we are to take a Darwinian version of the Law, it seems to me fairer to take the form, "nature abhors perpetual self-fertilisation," which my father adhered to in his later books.

An example of what seems to me the misuse of the Knight-Darwin Law occurs in my friend Mr. Willis' excellent book, "Flowering Plants and Ferns" (vol. i. p. 46). "In Myrmecodia, &c., Burck has found crossing absolutely prevented, the flowers never opening. Hence the Knight-Darwin hypothesis must be abandoned." If the abandonment of the hypothesis means the recognition of cases of apparently continuous self-fertilisation, the abandonment was made in 1868 by Darwin himself, as I have already shown. But Willis' abandonment seems to me part of an implied contention that Charles Darwin's generalisations are no longer a sufficient basis for floral biology. He seems to think that if the Knight-Darwin Law is not true, the fundamental principles underlying the study of the mechanism of flowers must be sought elsewhere than in Charles Darwin's works. In this point of view I think he is mistaken.

The attitude of the earlier writers towards the problem of cross-fertilisation seems, if I may venture to say so, to be elsewhere rather hastily treated by Willis (*loc. cit.*, p. 45). Take the following passage: "The advantages of cross-fertilisation are often great, and frequently enormous, and as at the first glance they appear to be obtained at little or no cost, we are inclined to expect this method of propagation to prove almost universal. The earlier workers at this subject in fact set out with the idea that cross-fertilisation was, so to speak, the primary object of a flower's existence, whilst self-fertilisation was actually harmful." Almost the whole of this seems to me to be unintentionally misleading.

That all "earlier workers" did not consider cross-fertilisation the primary object of a flower's existence, is shown by the following passage from "Cross- and Self-Fertilisation," p. 3: "We should always keep in mind the obvious fact that the production of seed is the chief end of the act of fertilisation; and that this end can be gained by hermaphrodite plants with incomparably greater certainty by self-fertilisation than by the union of the sexual elements belonging to two distinct flowers. Again, reviewing in 1876 ("Cross- and Self-Fertilisation," p. 8) his own treatment of the question in the "Fertilisation of Orchids" (1862), Darwin says: "I should have added the self-evident proposition that the propagation of the species, whether by self-fertilisation or by cross-fertilisation . . . is of paramount importance." Willis, therefore, seems to me completely wrong if he includes Charles Darwin among the earlier who considered cross-fertilisation the primary object of a flower's existence.

Nor, I think, is self-fertilisation ever treated by Darwin as positively harmful, though *perpetual* self-fertilisation is so treated. Self-fertilisation is constantly and correctly considered as less advantageous than cross-fertilisation—and in this sense (always bearing in mind the paramount importance of fertilisation of some sort) it may be said that self-fertilisation is relatively harmful.

Whatever may be the case with other naturalists, Darwin was certainly not inclined to expect cross-fertilisation to prove almost universal. Speaking of orchids, he says ("Fertilisation of Orchids," ed. i. p. 359): "Considering that the anther always stands close behind or above the stigma, self-fertilisation would have been an incomparably safer process than the transportal of the pollen from flower to flower. It is an astonishing fact that self-fertilisation should not have been an habitual occurrence." He saw clearly that plants pay a price for being so constructed that cross-fertilisation is possible; in fact, he saw that the evolution of the flower is the result of a gain and loss account between the advantage of cross-fertilisation and the risks and injuries consequent on the flower being open instead of closed, and therefore chasmogamic instead of cleistogamic. And this is in all essentials the theory which Willis ("Flowering Plants and Ferns," p. 46) gives as Macleod's, and proposes as a basis for floral biology, when the Knight-Darwin Law has been abandoned, and H. Müller's theory also given up. I am not able to read Macleod in the original Dutch, but it would appear from Willis's paper in *Science Progress*, 1895, that Macleod's contribution to the subject is full of valuable matter, but the essence of his theory (as given by Willis) seems to me to contain nothing with which my father was not familiar. What I object to is the tendency to condense Charles Darwin's contribution towards floral biology to a Knight-Darwin Law, and then, when the abbreviated statement does not explain everything, to abandon—not so much the law—but the general point of view which can only be gathered from Darwin's books as a whole.

The fact is that some modern biologist uses the Knight-Darwin Law in an inverted way, *i.e.* in a manner the reverse of Charles Darwin's way of using it. It was not to him a basis for the investigation of floral structures, but a generalisation extracted from that subject to serve as a foundation for the study of wider questions, such as the origin of sexuality. This is clearly shown in a passage from the first edition of the "Fertilisation of Orchids," where, after enunciating nature's abhorrence of perpetual self-fertilisation, Darwin goes on ("Fertilisation of Orchids," 1862, p. 359): "This conclusion seems to be of high importance, and perhaps justifies the lengthy details given in this volume. For may we not further infer as probable . . . that some unknown great good is derived from the union of individuals which have been kept distinct for many generations." H. Müller, perhaps, understood my father's use of the Law when he said ("Fertilisation of Flowers," p. 22) that the Knight-Darwin Law is not necessary for the elucidation "of the forms of flowers." But he would hardly have said as much of Knight's statement, that hermaphrodite flowers are adapted for intercrossing—which is the very foundation of the science of floral mechanism.

I now pass on to another writer—Knuth—who, in his useful "Blütenbiologie," seems also to be open to criticism in his treatment of the Knight-Darwin Law. In speaking of H. Müller's great work, he says ("Blütenbiologie," vol. i. p. 25): "The laws of Knight, Darwin, Hildebrand, Delpino gave no explanation of the numerous cases of efficacious self-fertilisation, nor of cleistogamy." Here Knuth does not seem to remember the conditions of thought under which the Knight-Darwin Law came into existence. As Loew ("Einführung in die Blütenbiologie," p. 143) has well said, self-fertilisation was formerly assumed to be the rule in hermaphrodite plants. In calling attention to the existence and importance of cross-fertilisation in hermaphrodites, Knight and Darwin assumed the existence of self-fertilisation. From the point of view of floral biology the important thing was the recognition of cross-fertilisation, and the law in which, unfortunately, this conclusion has been entangled need not "explain" the facts which the framers of the law assumed to be a part of common knowledge. With regard to cleistogamy, in its ordinary sense it is clear that there is no contradiction between the Knight-Darwin Law and the facts, as Loew has clearly pointed out ("Einführung," p. 144).

After the passage quoted above, Knuth goes on: "In the place of the one-sided law of the above-named naturalists (of which the general truth remains unproven) Müller set up a law, proved directly by Darwin's experiments and indirectly by the reproductive arrangements of plants in general, but especially by those of flowering plants. The law, namely, that 'when the offspring of cross-fertilisation come into serious conflict in the struggle for life with the offspring of self-fertilisation, the former (cross-bred) win the day. Only when this contest is absent can self-fertilisation suffice for reproductions for many generations.'"

I confess that this law is to me unsatisfactory. We ask ourselves "when is the struggle between cross- and self-bred offspring[1] absent?" Clearly when all the offspring are of one kind, *i.e.* all cross-bred or all self-bred: in a diœcious plant where all offspring are cross-bred, there is no question of self-fertilisation. In a plant with purely cleistogamic flowers, all offspring would be necessarily self-bred, and the law would imply that cleistogamic perpetuation may suffice. The law, therefore, amounts to this: that self-fertilisation will suffice only when it is unavoidable. This is as much as to say that any form of fertilisation is better than none. It is best to neglect this form of Müller's hypothesis, and to seek his meaning in his simpler and broader statements. In summing up his discussion in the "Historical Introduction," he says ("Fertilisation of Flowers," p. 23): "There is a good foundation, therefore, for the demand that the explanation of floral mechanisms shall rest only on the sufficient and demonstrable assumption that cross-fertilisation yields more vigorous offspring than self-fertilisation."

We have therefore as the chief points in Müller's theory:

(1) Fertilisation at any price.

(2) The increased vigour of cross-bred offspring.

Let us consider these more fully, and first for the conclusion that self-fertilisation is better than no fertilisation. This is a proposition which Müller has insisted on in the most interesting and instructive way, but it surely is not very novel in principle.[2] In a passage already quoted, Darwin reviewing, in 1876, his work of 1862 ("Cross- and Self-Fertilisation," p. 8), says: "I should have added the self-evident proposition that the propagation of the species, whether by self-fertilisation or by cross-fertilisation . . . is of paramount importance. Hermann Müller has done excellent service by insisting repeatedly on this latter point." No one had a higher respect than my father for Müller's work, and he had no disrespectful intention in describing Müller's contribution to the theory as self-evident. The interesting point is that these views did not strike him as original, because they had already occurred to himself.

That Müller based the explanation of floral mechanism on the experimental results of cross-fertilisation cannot be considered as a new departure. I should have imagined it to be notorious that this was Charles Darwin's view, if it were not that we find Knuth and others describing Müller's theory (in which this is the essential thing) as a great law of nature.

In a letter ("Life and Letters," iii. p. 291) to the late Asa Gray (September 10, probably 1866), Charles Darwin wrote: "I have seen the young seedlings from the crossed seed exactly twice as tall as the seedlings from the self-fertilised seed. . . . If I can establish this fact . . . in some fifty cases . . . I think it will be very important, for then we shall positively know why the structure of every flower permits, or favours, or necessitates an occasional cross with a distinct individual."

It seems to me that Charles Darwin's generalisations in regard to flowers may be summed up thus:—

(1) First comes what he called the self-evident proposition that fertilisation of some sort is of paramount importance. This is of the nature of an axiom.

(2) Then comes the direct observation that the vast majority of flowers are open. From this fact alone we should be justified in concluding that there is some advantage in cross- as compared to self-fertilisation, which advantage makes it worth while for flowers to run the risks and incur the expenditure necessarily connected with openness, and avoidable by cleistogamy. The innumerable adaptations for pollen-transport suggest and strengthen the same conclusion. But this is, properly speaking, only an elaboration of the fact that flowers are open.

(3) Direct experiment demonstrates the nature of this surmised advantage of cross-fertilisation over self-fertilisation.

As already pointed out, the Knight-Darwin Law in its usual form, *i.e.* no plant is self-fertilised *ad infinitum*, or in its improved form—"Nature abhors perpetual self-fertilisation"—is a generalisation drawn from observations on structure and experiments on crossing, the value of which in Darwin's opinion was rather its applicability to the problem of sex in a wide sense, than its use as a basis for understanding the mechanisms of flowers.

The point which seems to me important in the history of the subject, is that the above generalisations, which are in substance to be found in Darwin's works, are still the foundation-stones of floral biology, and would stand as firmly if the Knight-Darwin Law had never been formulated. For the naturalist who takes a wider field, and studies the origin of sex and the action of changed conditions, the existence or non-existence of perpetual self-fertilisation must always be an important question; but the law in which its non-existence is formulated, is not a fundamental canon of floral biology.

FRANCIS DARWIN.

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[1] I use the words *cross-bred* and *self-bred* to denote the offspring of cross- and self-fertilisation; we thus avoid the slightly obscure phrases cross-fertilised and self-fertilised seedlings which occur in Darwin's books.

[2] I am far from wishing to suggest that H. Müller's work does not contain much that is new and valuable; I am here considering only its fundamental bases.

BOTANY AT THE BRITISH ASSOCIATION.

THE subject of alternation of generations in plants played a prominent part in the work of the Botanical Section. The President (Prof. Bower) devoted a considerable portion of his address to the controversial questions connected with "the great enigma of the alternation of generations" in green plants. Mr. Lang, of Glasgow University, and Prof. Klebs, of Halle, contributed important papers on this subject, and these were followed by a general discussion on the problems of alternation. Mr. Lang gave an excellent summary and critical review of our present knowledge concerning alternations of generations in the Archegoniatæ. The recent work of this investigator on some striking cases of deviation from the normal life-history of ferns, must be ranked among the most important contributions germane to this subject which have appeared in recent years. In concluding his account of some of the main factors in alternation, the author suggested three subsidiary questions as worthy of attention—the probable line of descent in archegoniate plants, the bearing of the cytological facts on the question, and the significance to be attached to apospory and apogamy.

Prof. Klebs' paper dealt with the alternation of generations in the Thallophyta, a subject which he was particularly well fitted to discuss from a critical standpoint. After taking a general survey of the various divisions of the Thallophyta, Prof. Klebs referred more especially to certain cases which have a more direct bearing on the question of the first appearance of a regular alternation of generations. The majority of the Algæ and Fungi have two or more kinds of propagation, each of which necessarily depends upon definite external conditions. According to the conditions the different kinds of propagation may appear on the same or on different individuals, independently or in any succession. The fertilised ovum in sexual forms does not differ essentially on germination from another propagative cell. In none of these cases is there any reason for speaking of an alternation of generations. In conclusion, the author briefly referred to the possible connecting links between the Algæ and Archegoniatæ. Sir Edward Fry, Dr. Scott, Profs. Marshall Ward and Marcus Hartog took part in the discussion which followed the two contributions by Mr. Lang and Prof. Klebs.

Another important item in the programme of Section K was a semi-popular lecture by Dr. F. F. Blackman, on the breathing mechanism of plants. The lecturer gave a clear and interesting summary of the progress of experimental work on the phenomena of gaseous exchange between a green plant and the medium in which it grows, concluding with an account of some recent investigations which have not yet been published.

Algæ and Fungi.—The Committee on Fertilisation in the Phæophyceæ reported very satisfactory progress in the researches on the Fucaceæ and Dictyotaceæ. Mr. Lloyd Williams, of Bangor, whose researches have been carried out under the auspices of the Committee, gave an account of his important work on the reproduction of *Dictyota dichotoma*. *Dictyota*, an annual brown seaweed, germinates during the